VOLUME LII, No. 8 APRIL 14, 1955

THE JOURNAL OF PHILOSOPHY

WITTGENSTEIN ON SOME QUESTIONS IN FOUNDATIONS OF MATHEMATICS

"PHILOSOPHY is a battle against the bewitchment of our intelligence by means of language."[1] This pronouncement in Wittgenstein's posthumously published book is an index to the philosophical outlook which prompted Wittgenstein to scrutinize with the greatest attentiveness the language in which philosophers have stated their problems. Each problem is according to him the product of an obsession—a linguistic obsession that is not recognized.[2] In consequence of this the philosopher envisages his task not as the elimination of the obsession, but rather as the solution of a scientific problem. It is as if he had to find out something new, as if he faced a question of fact about which we do not yet know enough.[3] "The real discovery," Wittgenstein says, "is the one that makes me capable of stopping doing philosophy when I want to.—The one that gives philosophy peace, so that it is no longer tormented by questions which bring *itself* in question."[4] The sign that this discovery has been made is that we cease to seek a *solution* of a particular philosophical problem. These problems are not the kind that have a solution, in the usual sense of "solution." They should *dis*solve, "*completely* disappear,"[5] once clarity about our use of language is achieved.

Rather than launch into an extended discourse on method I shall try here to exhibit Wittgenstein's procedure in dealing with certain philosophical problems, and I shall at the same time expound the substance of what he had to say about them. The problems I have chosen come from the foundations of mathematics. They were treated by Wittgenstein in lectures I attended in Cambridge in 1934–35 and in the 1939 lectures on foundations of mathematics of which I possess notes. Initially I shall set a question which Wittgenstein did not formulate in precisely my fashion; but my formulation provides a springboard for the exposition of his treatment of problems intimately connected with it.

[1] *Philosophical Investigations* (New York, Macmillan, 1953), p. 47.
[2] Lectures, 1934–35.
[3] Ibid.
[4] *Philosophical Investigations*, p. 51.
[5] Ibid.

I shall begin with a paraphrase of Kant's question about pure mathematics: How is applied mathematics possible? How is it that *a priori* propositions have an application to matter of fact? The puzzle suggested by this question is, more specifically, the puzzle as to how an *a priori* proposition, e.g., $2 + 3 = 5$, can both be true independently of matter of fact and be true of collections of two apples and three apples. If *a priori*, such that its truth-value is unaffected by any theoretically possible state of affairs, it can give no information about any actual state of affairs. How then can it be about apples? Puzzlement about this may very well have been one source of Mill's denial that "$2 + 3 = 5$" is anything more than an empirical generalization. If it cannot both be true regardless of fact and also imply a truth about apples or other observable objects—in particular about objects which do not coalesce or reproduce themselves in the course of being counted—one must discard one of the two seemingly incompatible accounts of it. Mill discarded the account of it as a necessity. And yet the arithmetic statement "$2 + 3 = 5$" seems obviously to possess all the properties ascribed to necessary truths: it can be known without recourse to experience; its opposite would be self-inconsistent; no state of affairs could possibly disconfirm it, nor would any be required to confirm it. How then can one account for the harmony between the two quite different areas of logic and of empirical fact? How is it that we can apply arithmetical calculations to physical objects, or trigonometric calculations to physical lines and angles? Is there a genuine mystery here or only a gratuitous puzzle?

The suggestion which my question makes, unlike that which Kant intended by his, is that there is a difficulty in conceiving any application of a proposition of logically incorrigible status to matter of fact—that application is impossible. The question thus has what Wittgenstein singled out as the earmark of every philosophical difficulty: the presence in its expression of the words "cannot" or "must," or their equivalents. These are the words which signalize a philosophical obsession. How, we ask, *can* the statement "$2 + 3 = 5$," whose truth is independent of experience, apply to apples, i.e., be such that the numerical equality it asserts not only tallies with, but seems to be empirically established by, a count of the members of the two sets of apples, and seems even to *predict* the empirical result of counting? As Russell said, "We do not know who will be the inhabitants of London a hundred years hence; but we know that any two of them and any other two of them will make four of them. This apparent power

of anticipating facts about things of which we have no experience is certainly surprising."[6]

But the applicability of mathematics is not surprising to common sense. That arithmetic, geometry, and trigonometry have an application is a commonplace, and no philosopher in his ordinary pursuits questions whether mathematical propositions can apply to matters of fact any more than he questions whether motion is possible. But one cannot as a philosopher dismiss the question by an appeal to common sense. The common-sense answer to "How can '2 + 3 = 5' imply a truth about collections of apples?," namely, "It simply does," is true; but it is not the proper answer to the philosophical question. The proper answer should rid one of the puzzle. This, says Wittgenstein, is the business of philosophy: to rid one of puzzles which do not arise for common sense.[7] Doing philosophy according to him consists of three activities: first, seeing the common-sense answer to these problems; second, getting oneself so deeply into the problems that the common-sense answer seems unbearable;[8] and finally, getting oneself from that situation to the common-sense answer again. But the common-sense answer by itself is no solution; one must first allow oneself to be dragged into the mire and then get out of it.[9]

Were it proper to describe an arithmetic proposition as an empirical generalization having no exceptions, then its application to fact would present no puzzle. But tempting as it is to escape a difficulty in this way, I think it is clear that we should do violence to the current usage of the term "empirical generalization" were we to take this way out. For an empirical generalization can be falsified, and it is clear that we will accept nothing as a counter-instance to "2 + 3 = 5." Mill's theory re-classifies arithmetic propositions, and furthermore, in such a way as to leave us with no proper use of the word "necessary."[10] For if arithmetic propositions are not necessary, we are at a loss to describe what would be necessary, just as we should be at a loss to say what would be a religious belief if the description "religious belief" were refused to "There is a God." Wittgenstein says

[6] *The Problems of Philosophy*, p. 132.

[7] Lectures, 1934–35.

[8] In this connection C. D. Broad's comment on common sense is worth remarking: "Let it go out and hang itself" (*The Mind and Its Place in Nature*, p. 186).

[9] Lectures, 1934–35.

[10] See M. Lazerowitz, *The Structure of Metaphysics* (Routledge & Kegan Paul), pp. 258–259.

that what he does under the name "philosophy" "may in no way interfere with the actual use of language."[11] "It is not our aim to refine or complete the system of rules for the use of our words in unheard-of ways."[12] "What *we* do is to bring words back from their metaphysical to their everyday usage."[13]

Let us begin then with acceptance of two facts: (1) that it is proper to describe mathematical propositions as necessary, and (2) that applied mathematics is possible. Whatever the philosophical difficulties involved, these are the facts which common sense dictates that we begin and end with. But philosophical difficulties in which it is easy to become mired do exist. I shall try to expound these difficulties as Wittgenstein envisaged them, together with the attempts he made to clarify them and, by clarification, to dissolve them. These are all intimately connected with whatever problem may be felt about the possibility of applied mathematics, though Wittgenstein did not make this particular problem central in the cluster of related problems he investigated. All of them concern the connection of mathematical propositions with experience, and in my opinion it would not be a misrepresentation of Wittgenstein to say they all arise directly or indirectly from the misleading question, "What are mathematical propositions *about?*"

It has sometimes been held that "2 + 3 = 5" is a proposition about numbers, necessarily true in virtue of the nature of numbers, whereas "2 apples + 3 apples = 5 apples" is a proposition about apples, which is factually true in virtue of the nature of the apples our world provides—non-generating, non-coalescing apples. We can use arithmetic to count, and to predict the result of adding two apples to three, because, so it is claimed, it is a fact about apples that they do not either vanish or multiply when this operation is performed. The application of arithmetic thus depends on whether or not certain empirical conditions are satisfied. It will be true then to say "If no apples disappear or multiply, 2 apples + 3 apples = 5 apples," but not unqualifiedly true that 2 apples + 3 apples = 5 apples. Now Wittgenstein says that whether this is a correct account of the proposition "2 apples + 3 apples = 5 apples" is to be determined by the *use* we make of it. It is not that the use is determined by whether the proposition states a contingent truth about apples or a necessary connection between concepts. If, unexpectedly, apples increase or diminish in number when addition is performed, and we accept

[11] *Philosophical Investigations*, p. 49.
[12] Ibid., p. 51.
[13] Ibid., p. 48.

this fact as constituting a falsification, then our statement is experiential. But if we excuse every case in which five apples fail to be present when three apples are added to two, i.e., if no such fact is accepted as disconfirming it, then our statement is necessary. One and the same sentence can be used in either of these two ways, and of course it is a fact that the latter way is by far the more usual.

If we examine this more usual use of "2 apples + 3 apples = 5 apples," i.e., to express a necessary proposition, we shall see its proper relation to the empirical fact that apples remain discrete when added. This fact is not an empirical condition of the truth of the proposition it is used to express; rather, that this proposition applies to apples is the criterion for their having remained discrete. And if in an imaginable case it did not apply, i.e., if the number of apples counted was not five, this would be the criterion, not for the equation's falsity, but for the number of apples not having remained constant during the process of their being counted. Similarly, that the equation "2 quarts + 3 quarts = 5 quarts" does not hold for the *physical* addition of two quarts of alcohol to three quarts of water indicates something about the mixture of these substances, but the behavior of these substances when mixed implies nothing about the truth of "2 quarts + 3 quarts = 5 quarts." Their behavior does imply the falsity of the statement "2 quarts physically added to three quarts yield five quarts," but it is logically irrelevant to the statement which asserts the arithmetic addition of units—as our usual use of this statement shows.

One likely source of the temptation to disregard how such a statement functions (as necessary rather than empirical) is the compulsion to ask, and to answer, the question, "What is the proposition about?" "About apples," "about discrete entities," etc., are the natural answers. Similarly, "about numbers" is the natural answer to "What is '2 + 3 = 5' about?"; and of course it cannot be denied that it is proper to distinguish this proposition from empirical propositions by characterizing it as being about numbers. But our question, according to Wittgenstein, is misleading, since we thereby treat "2 + 3 = 5" as analogous to empirical propositions, and only differing from them in being about non-empirical, abstract entities. Mathematics, according to this way of looking at it, becomes a sort of physics of mathematical entities, and mathematical research an expedition of discovery. This is the conception which Professor G. H. Hardy had. He writes: "I have myself always thought of a mathematician as in the first instance an *observer*, a man who gazes at a distant range

of mountains and notes down his observations. His job is simply to distinguish clearly and notify to others as many different peaks as he can. . . ."[14]

Wittgenstein says that philosophy arises out of prejudices in favor of certain grammatical forms. We try always to work from one paradigm, which operates as a grammatical obsession.[15] "What are mathematical propositions about?" is a question motivated by the obsessional emphasis on the analogy of these propositions to empirical ones. To rid one of this obsession it has some point to say, as Wittgenstein says in a number of places, that arithmetic propositions are not about numbers, nor are geometric propositions about geometrical figures. But this is also misleading, since, like Plato's answer to this question, it seems to give information in the way in which a scientific answer does. If we wish not to be misled we shall do well to direct our attention away from the question as to what they are about to the *use* we make of them. And by examining their use the connection between their necessity and their application will no longer appear puzzling.

Now what we do when we allow nothing to count against a proposition, when we enshrine it amongst the incorrigibles and refuse to surrender it in the face of any conceivable facts, is to assign to the expression for the proposition a special rôle in our language. According to Wittgenstein we have decided on using the sentence in a certain way, namely, as a rule for the use of expressions, i.e., a rule for the application of certain words.[16] To elucidate, let us consider an example similar to one he used: Suppose I multiply 25 by 17 in order to find out the number of squares in a rectangle 25 squares long by 17 squares wide. If the number of squares is found upon counting not to be 425, the result got by multiplying according to the rules, and I thereupon say "$25 \times 17 = 425$" is false, I use it to express a proposition testable by experience. But if I say it is correct regardless of what number of squares I find on counting, and use it as a criterion of the correctness of my count, I thereby make it independent of experience. And to do this is to resolve on a certain use, namely, that it shall function analogously to a rule for the use of numerical terms—for one thing, that 425 can be substituted for 25×17, for another, that the two statements, "The number of roses I received is equal to the quotient 425/17" and "The number of roses I received is two dozen," may not describe the same fact. Similarly, the statement that it is impossible to con-

[14] "Mathematical Proof," *Mind*, Vol. XXXVIII (1929), p. 18.

[15] Lectures, 1934–35.

[16] Lectures, 1939.

struct a heptagon with straight edge and compasses functions as a rule which prevents my saying with sense, "I drew a heptagon on the board using only straight edge and compasses."

Mathematical propositions are *preparations* for the use of language, says Wittgenstein, almost as definitions are.[17] Note that he does not say they *are* either definitions or statements about symbols; but they function as explicitly formulated linguistic rules in fact function. Euclid's proof that a line can be bisected by a certain method serves to provide a rule for the application of "equal lengths," and arithmetic serves to give rules for the use of number words, whereas no non-verbal empirical proposition ever functions analogously to a rule governing the use of language. If, then, the function of mathematical propositions is to govern usage it is no more surprising that they have an application than that a knife should cut. The connection between them and their application is like that between a rule for the use of an expression and the occurrence of that expression in various verbal contexts.[18] For example, "exactly one straight line can be drawn between any two points" functions prescriptively: in understanding it we know it makes sense to say that one physical straight line, but not to say that more than one, is drawn between two points. One tends to look upon the geometrical proposition as asserting a truth about ideal lines which somehow also holds of coarse drawn lines. But the application of a geometrical proposition in an experiential context is to show, not what is true or false, but what makes sense or nonsense.[19] It obscures an important difference between empirical generalizations and their purported instances and mathematical propositions and the things to which they apply to say that both kinds of propositions show what is true, or false. Necessary propositions about ellipses and circles show that "I cut an elliptical cake in eight equal parts" does not make sense, whereas "I cut a round cake in eight equal parts" does. The relation of the necessary propositions about circles to the physical circle is like that between a rule and its application, not between a generalization about ideal circles and a rough approximation.[20]

This account squares with the fact that mathematical propositions do not get confirmed or disconfirmed by experience: one does not confirm or disconfirm by seeing, feeling, etc., a proposi-

[17] Ibid.
[18] Ibid.
[19] Lectures, 1934–35.
[20] Ibid.

tion whose use is to show what makes sense rather than to assert what is in fact true, or false. Further, there are certain puzzles about the connection of mathematics with experience which this account helps clarify. One is that mathematical propositions when applied seem to make predictions whereas, being necessary, they cannot. Suppose we say that two crystals which separately weigh three grams each must together weigh six grams, or that six two-foot boards must fit into a space twelve feet wide. If these are predictions, then they can be false. It could happen that six two-foot boards cover more or less then twelve feet, and that the two crystals weigh more or less than six grams. But "$6 \times 2 = 12$" and "$3 + 3 = 6$" are not predictions. They function as criteria for judging when the boards do not fit or the scales read five, that something *must* have happened.[21] "$6 \times 2 = 12$" does not even assure us that, unless the boards change, six two-foot boards will fit into twelve feet, for the criterion of change is their not fitting. If we say they *must* fit, and cite "$6 \times 2 = 12$" as evidence while at the same time refusing to accept any other method of showing the width of the boards, then, says Wittgenstein,[22] we are not saying anything about measurement. The burden of what we are saying is that what is called two feet is what goes six times into twelve. Were we making an experiment to determine whether six such boards will fit the space, the result would not be fixed in advance, and prediction of the result would be appropriate. The difference between a mathematical calculation and an experiment is that in fixing the rules of the calculation one fixes the result.[23]

What Wittgenstein says about the nature of mathematical propositions also has a bearing on the further puzzle, namely, that we sometimes seem to discover a fact of experience which we then go on to prove must be so. It looks as if a matter of fact has an *a priori* demonstration. Pythagoras' theorem is a case in point. But the puzzle is gratuitous. According to Wittgenstein what happens is that an empirical proposition, which experience discovers, is converted into a proposition which no experience could make us give up. The proposition which is made independent of experience is suggested by experience. For example, the proposition, "A pentagram is a pentagon plus five triangles," is certainly suggested by experience, but it functions as a rule because we allow no method of construction to invalidate it. To see a pentagram as this composite is an experience, but as Wittgen-

[21] Ibid.
[22] Ibid.
[23] Lectures, 1939.

stein says, there is no comparable process of seeing that a rule holds.[24]

Now what may appear as a surprising harmony between mathematical propositions and their application is merely due to our assigning the functions of a rule to those propositions which conform to fact, other things being equal.[25] Because the specific gravity of iron is 7.86 it would be natural and might be useful to say "No matter what experiment shows, the specific gravity of iron is 7.86." By this token an empirical proposition would be given a status and function like that of a rule of language. But the matter of fact does not compel acceptance of this change of status. The kind of fact which persuades us to accept it is that it is useful; and that it has applications is a mark of its usefulness. Thus, if we had a world in which counting the members of two groups having 2 members and 3 members, respectively, *never* totaled up to 5, some other proposition than "$2 + 3 = 5$" would have been adopted as necessary. If circumstances made it practical to calculate differently than we do, e.g., because things multiplied or disappeared regularly upon being counted, we should adapt arithmetic calculation to the circumstances.[26] If we got different results every time we counted the squares of a rectangle 17 by 25, we should probably not say the calculation, 17 multiplied by 25, was a proof that $17 \times 25 = 425$. We might still call the calculation a piece of arithmetic just as "it is not the case for all a and b that $a \times b = b \times a$" is a part of group theory. But we should either have different arithmetics, or we should have an arithmetic in which certain multiplications had different results. For example, as our world is now, there is no phenomenon for which "$23 \times 18 = 800$" has any use. But if these numbers were constants relating to all natural phenomena, says Wittgenstein,[27] we could imagine an arithmetic in which this multiplication, among all others, had two results. To the objection that it is in the nature of 23 and 18 to give 414, he replies that in giving a rule of multiplication we do not give an infinity of applications of it. Behind the use of a rule is a habit of reacting in a certain way. Given the rules of multiplication we do in fact agree in getting the result 414; to do this is natural. And it is this fact which makes us say this result is correct. But we can imagine having always agreed in getting 800. And if this were in fact the case, would not 800 be the *correct* result? What

[24] Lectures, 1934–35.
[25] Ibid.
[26] Lectures, 1939.
[27] Lectures, 1934–35.

would it be like to say that we always had made a mistake in thinking $23 \times 18 = 800$? Our agreement, not that such and such a result *is* the case, but *in getting* that result, is what determines what is called a correct calculation. Thus arithmetical propositions, though independent of experience, are in two ways dependent upon experience: in being suggested by experience, and in having their special function rest on common linguistic habits.[28]

In the remainder of this paper I should like to examine certain things which Wittgenstein appears to be saying concerning the connection between the necessity of mathematical propositions and their origin in and application to matter of fact. He seems to be saying that it is by an arrangement of ours that, for example, the arithmetic proposition $2 + 3 = 5$ tallies with the empirical result of counting two groups of 2 and 3 things, respectively, and that if circumstances were different, so that counting the members of such groups never resulted in 5, we should adapt counting to the circumstances and accordingly have a different arithmetic. That is, if circumstances were different, we should have a different necessary proposition. A proposition can be "suggested by experience and then made independent of experience."[29] Thus an expression which is given a special place in our language by being used to denote a necessary proposition is somehow connected with fact. I should like to examine both the hypothesis and the consequent of the statement, "If circumstances were different we should have a different necessary proposition"; for there is an unclarity about both.

How are we to understand the words "circumstances such that the members of two groups of 2 and 3 things, resp., never total up to 5"? One possible interpretation of these words, though I advance it with hesitation and without intending to imply it was Wittgenstein's, is the following: that the juxtaposition of two groups of objects, each of which we correctly counted as having 2 and 3 members, respectively, should result in the creation or destruction of, say, one individual, so that subsequent counting of the combined sets showed more, or fewer, than 5 objects. It is of course perfectly conceivable that something like this should happen. Wittgenstein has said[30] that if, for example, things disappeared regularly in certain ways it might be practical to count differently, that one might adapt one's technique of counting to

[28] Ibid.

The expository account given thus far constitutes a paper presented to the American Philosophical Association, Eastern Division, December 29, 1953.

[29] Lectures, 1939, 1934–35.

[30] Lectures, 1939.

the circumstances. Our arithmetic might then include the statements "2 + 3 = 4," "1 + 1 = 1," etc. That is, we should adopt these as expressing necessary propositions, so that in the exceptional case when our final count was 5, or 2, we should say not that these propositions were false, but that the objects must have reproduced, just as now we say, when one object put in juxtaposition with another results in one, "the objects must have coalesced," or "one must have vanished." That we should say this sort of thing is a sign that we are in the two comparable cases taking "2 + 3 = 4" and "2 + 3 = 5," respectively, to be necessary: we accept nothing as a falsification.

Throughout this paper it will be noted that I am interpreting Wittgenstein to be taking arithmetic to consist of what are commonly called necessary propositions, i.e., propositions that are both non-empirical and *true*. The problem is to explain the connection of such propositions with matter of fact, with their application. Professor G. E. Moore, relying on lectures of the period 1930–33, points out [31] that Wittgenstein characterized "2 + 3 = 5" and the like as "rules of grammar," "treating only of the symbolism," and as being neither true nor false. This Wittgenstein undeniably did; "2 + 3 = 5" was said to be a rule specifying a possible manner of speaking or writing (which one might adopt or not). But he also stated at various times that "2 + 3 = 5" is not a definition, nor *about* the symbolism in the way "'2 + 3' is interchangeable with '5'" is about the symbolism, although it is used analogously to the way we use such a rule. For example, it prevents our saying such things as "I augmented my savings of three hundred dollars by two hundred more but did not have a total of five hundred." Moore thought he might be using such an expression as "2 + 3 = 5" in two different ways, to express a necessary proposition and to state a rule for using words or sentences, and even that when he used it in the first way it expressed something neither true nor false.[32] But it is obviously self-contradictory to describe necessary propositions as being neither true nor false; and one can find support in Wittgenstein's lectures (e.g., in his comparison of necessary and empirical propositions) for his supposing them to be *true*, though of course not true in the sense in which an empirical proposition is true. It may be no consistent account is to be had. I am going to assume that though he holds that such expressions as "2 + 3 =

[31] "Wittgenstein's Lectures in 1930–33," II, *Mind*, Vol. LXII (1954), no. 251, pp. 298–308.

[32] Ibid., p. 302.

5'' are *used* to proscribe certain linguistic combinations,[33] he also holds that they do nonetheless express necessary truths, and hence that in a world of coalescing objects what is expressed by ''2 + 3 = 4'' is a necessary truth.

Now in this hypothetical world is it proper to say we should have a different *arithmetic*—in particular, that *arithmetical* addition of 2 and 3 would yield a different result than 5? We can easily conceive of ''2 + 3 = 4'' expressing a necessary proposition (it is easy to conceive of the involved symbols being used differently), but if we use ''2,'' ''3,'' and ''4'' as we do now, surely the meaning of some other symbol or symbols must change. The natural assumption is that ''+'' can no longer mean arithmetic addition of two numbers, nor ''='' arithmetic equality. ''2 + 3'' must denote not the arithmetic sum of two numbers, but the physical combination of two sets, and ''='' must mean something like ''yields.'' ''2 + 3 = 4'' would be a shorthand for ''(x). $2x + 3x = 4x$,'' interpreted as ''2 things physically conjoined with 3 yield 4.'' But it describes this world paradoxically to say its arithmetic is different from ours. ''2 + 3 = 4'' only appears shocking if taken to express an equality between a number and an arithmetic sum of numbers, as it does now. If ''+'' had a different usage in this hypothetical world, and if we also had our arithmetic for sums of numbers, the expressions ''2 + 3 = 4'' and ''2 + 3 = 5'' both could without inconsistency express necessities, although confusion might result. And if there were but one arithmetic, what we might call the arithmetic of invariant coalescence, it is misleading to say that we have made ''4'' and ''2 + 3'' interchangeable as though *in preference to* ''5'' and ''2 + 3,'' since ''2 + 3'' has two entirely different uses when equated with ''4'' and with ''5''. The sentence ''In different circumstances some other proposition would be necessary'' suggests that were facts different we should arrange that the *addition* of 2 and 3 would necessarily yield a different result. This is unobjectionable if we reinterpret ''addition.'' What Wittgenstein says is then little more than that in a different world we might have a different language. What is interesting about his statement is the claim that a difference in the language of arithmetic is influenced by the exigencies of making application of arithmetic to fact.

There is reason to suppose that what I have described as possibly illustrating a circumstance in which we might have a different arithmetic, and the account I have given of it, is not in

[33] Note that such might be their use without their translating into rules ''treating of the symbolism.''

fact what Wittgenstein had in mind. It will be worthwhile to consider an example which Professor Moore reports in Wittgenstein's 1930–33 lectures,[34] in order to note differences and to elaborate what Wittgenstein has said. In this example I think it is clear that "+" continues to be interpreted as arithmetic addition, however puzzling this may be; and this is the main respect in which it differs from the case I have discussed. I shall alter the numerals so as to make comparison easier. Wittgenstein supposes the following imaginable circumstances: (1) that one has the two experiences of counting first up to 2, then up to 3 in the case of two groups of apples, and (2) then a third and subsequent experience of (correctly) counting *all* the apples and finding only 4. This is imaginable because it is a mere matter of experience that one usually finds 5, inasmuch as apples do not vanish without cause. "2 + 3 = 5" makes no prophecy as to what experience one *will* have upon counting *all* the apples. But if one were to find 4 apples, the most natural comment to make would be that one must have vanished. By this comment Wittgenstein says we can only mean "If we keep to the arithmetical rule '2 + 3 = 5' we have to say 'one must have vanished.'" In analyzing this latter statement and what he takes to be its consequences Moore augments the circumstances (1) and (2) above by two further ones which he supposed Wittgenstein to have had in mind as the situation in which one made the comment "one apple must have vanished": (3) that one knows, because one has kept watch, that nothing has happened to account in any normal way for there being only 4, (4) that one does *not* know, by counting done by oneself or by someone else, that one has counted out a total of 5, so that if one said there were 5 apples in all, this would be a deduction from the fact that one had counted out 2 + 3 of them. It is important to keep circumstance (3) in mind in appraising the consequences Moore draws from his analysis of Wittgenstein's claim that by "one must have vanished" we can only mean "If we keep to the rule '2 + 3 = 5' we must say 'one must have vanished.'"

Suppose one says there are 5 in all. Moore claims that Wittgenstein's reason for explicating "one must have vanished" as he did, is that "there are 5," if asserted under circumstances (1) and (4), means something different from what it would mean had one discovered by counting, rather than deduced, that there are 5, namely, B: "One keeps to the rule '2 + 3 = 5' if one asserts there are 5 apples and violates it if one asserts anything incon-

[34] Loc. cit., pp. 302–304.

sistent with saying there are 5."[35] And this is the only thing meant by "there are 5" in circumstances (1) and (4). Now one can keep to the rule, i.e., speak correctly, without saying what is true: it can be correct but not true to say there are 5. Moore concludes that Wittgenstein's insistence on proposition B as the only thing that we mean by "there are 5" is intended to prevent the mistake of supposing we mean "If one sets out 2 + 3 apples then *necessarily* one sets out 5." This proposition Moore takes Wittgenstein to suppose is false, which is to say he supposes it imaginable that one should count 2 + 3 apples and that a correct count of the total *at that very time* should show only 4. Further, Moore thinks that whether or not this interpretation of Wittgenstein is correct, it is quite certain that he held that "2 + 3 = 5" is never used in arithmetic to express a proposition from which it follows that if one counts out 2 + 3 apples one necessarily counts out 5.[36] In this case my conclusion would be that neither "2 + 3 = 5" nor "If one has 2 + 3 apples one has 5 apples" is a necessary proposition.

Wittgenstein has at various times certainly said things which support the account Moore has given, e.g., that "2 + 3 = 5" is "purely arbitrary," which suggests that there is no necessity about "If one counts out 2 + 3 apples the total is 5." I am not now in a position to judge whether this account of what Wittgenstein held is in fact correct, although I heard the 1932–33 lectures in which there was some discussion of the example under consideration. Nor for that matter am I sure that I am correctly reporting what he said in the lectures I heard in 1934–35 and in later lectures to which I have access at second-hand. If Wittgenstein did commit himself to holding it to be imaginable that one should count out 2 + 3 apples and not at the same time have a total of 5, I think, with Moore, that he was surely mistaken. If he did, then "If one counts out 2 + 3 apples one has a total of 5" would be an empirical proposition. But I wish to make plain that I have not supposed him to imply this and my criticism will not presuppose thinking he does.

For one thing, (a) when Wittgenstein said that "2 + 3 = 5" functions analogously to a rule of language, i.e., so that "I counted out 2 + 3 apples but did not at that time have a total of 5" does not make sense, I took it that what does not make sense could not express an imaginable state of affairs. Further, the reason for its not expressing an imaginable state of affairs is that the truth of "the total is not 5 apples" is inconsistent with the truth of "I

[35] Ibid., p. 306.
[36] Ibid., pp. 307–308.

counted out 2 + 3." That is, if I did count out 2 + 3 apples it necessarily is *true* that I counted 5. It is not merely that I must engage in this manner of speaking (and say there are 5) if I am to speak in accordance with a rule. This certainly seems to me to be correct, although I am not at all sure but that Wittgenstein held what Moore reports.

For another thing, (b) what makes convincing Moore's concluding that Wittgenstein held to be imaginable a logically inconceivable state of affairs is circumstance (3) of the example. (3) is to the effect that it is *known,* because one has kept watch, that nothing has happened to account in any normal way for there being only 4 apples, e.g., it is known that none has been removed, or has flown away. Only if Moore means that knowing this implies that *none has vanished* could it be inferred that one could set out 2 + 3 apples which total up to 4. But if we *know* that none has vanished, should we say, on counting 4, "one must have vanished"? I doubt that Wittgenstein intended this circumstance to figure in the example he was considering; rather, it seems to me that he supposed not that one knew an apple had *not* vanished, but that one did *not* know that it had, although it in fact had, by some process quicker than sense-observation could detect. When Moore says he can imagine that one really has vanished, even under circumstance (3),[37] it appears that he also is holding not that one knows none has vanished but that one merely knows none has vanished *in any normal way,* though one has in fact done so. But then it could not be inferred from Wittgenstein's example that he held that one could set out 2 + 3 apples and have *at that time* 4. Rather, if one set out 2 + 3 apples and one vanished, one would have 4. And then we should have our present arithmetic—unless "2 apples + 3 apples = 4 apples" were interpreted as a statement about physical combination.

However, Wittgenstein made a comment on the example, according to my lecture notes of 1932–33, which might well have led Moore to suppose that Wittgenstein is committed to holding it to be imaginable that one should set out 2 + 3 apples and have a total of 4. The comment was that in circumstances (1) and (2) we can *either* say "one must have vanished" or we can *"change the rules."*[38] That is, we can choose either to say "I set out 5 apples" (and thereby speak in accordance with the present rule "2 + 3 = 5") or to adopt the rule "2 + 3 = 4"—and accordingly

[37] Ibid., p. 309.

[38] Here is a clear case of Wittgenstein's characterization of "2 + 3 = 5," etc., as rules. I shall use his language in expounding what he said.

speak correctly in saying "I set out 4." Moore took it that since we can speak correctly without saying what is true, it could conceivably be false that one set out 5 apples when one set out 2 + 3. Wittgenstein's insistence that "there are 5" only means "If the rule '2 + 3 = 5' is adhered to one must say one put 5," Moore took to indicate that a proposition commonly held to be true was false, namely, "If one sets out 2 + 3 apples *necessarily* one sets out 5." And if this is false it would seem that one could set out 2 + 3 apples and not have a total of 5.

I have placed a different construction on what Wittgenstein said. Because he held that as language is used "I set out 2 + 3 apples but did not have a total of 5" does not make sense, I am supposing (as Moore does not) that he held that "2 + 3 = 5" is used in arithmetic to express a proposition from which it follows that if one set out 2 + 3 apples one necessarily sets out 5, that is, that if "2 + 3 = 5" is a necessary proposition so is "If I set out 2 + 3 apples I have a total of 5." And the latter *is* necessary because "2 + 3 = 5" expresses a necessity, as we use language now. When Wittgenstein says that under circumstances (1) and (2) one can either say "one must have vanished" or change the rules, I take him to be saying that either we can keep to our present arithmetic, whence "If I set out 2 + 3 apples I have 5" would be necessary, or we can have a different arithmetic, whence, for example, "If I set out 2 + 3 apples I have 4" would be necessary. Thus with present arithmetic "I counted out 2 + 3 but did not have a total of 5" would express a self-contradiction, and *with a different arithmetic* it would be a redundancy expressing a possible state of affairs. But what is expressed would be contingent on which arithmetic we chose. Now it is rather different to think (as I believe Moore did) that Wittgenstein is committed to holding it to be possible that one should set out 2 + 3 apples and have only 4 and to think him committed to holding that it would be possible *if* our arithmetic were different. It must be admitted, however, that the one position seems no whit better than the other. The example seems rather clearly to use "2," "3," "4," "+," and "=" precisely as we do now: the numerals to stand for the numbers we correlate with a couple, a trio, and a quartet, "+" to mean addition (not physical conjunction, as in my first interpretation), and "=" to mean "equals." But if this is their use it is difficult to know what could possibly be meant by saying "2 + 3 = 4" is necessary, or by saying that if our arithmetic were different (e.g., if this proposition were necessary) then it would be necessary that if I set out 2 + 3 apples I should have a total of 4.

The unclarity about this matter makes it unclear what is meant by saying that depending on the circumstances different propositions would be necessary. I should like now to examine this claim together with the view Wittgenstein apparently held that it is a matter of choice whether or not "2 + 3 = 4" expresses something necessary (whether we have "a different arithmetic"). According to Wittgenstein *we adopt* necessary propositions, and which ones we adopt is "suggested by experience." Present circumstances are such that we deduce "I set out 5 apples" from "I set out 2 + 3"; but we could choose to deduce "I set out 4 apples" instead. The fact that the arithmetic in use tallies with the result of counting is presumably explained by our choice being suggested by experience. Facts do not compel the choice, but they suggest it. Other things being equal, we take as necessary the proposition conforming to fact.[39] For example, were we to say "The specific gravity of iron is 7.86, no matter what experiment shows," we should thereby make independent of experience a proposition which experience suggests. Had we a different arithmetic presumably the difference would be explained by what is suggested by experience: in our example, by the experience of always finding 4 apples when one counts out a couple and a trio.

But now *what* is suggested by experience? That it would be useful to adopt *these propositions* as necessary? *These* propositions are first of all factual truths, empirical propositions. Hence *they* cannot be made independent of experience, i.e., *these* propositions cannot be necessary. One and the same proposition cannot depend for its truth or falsity on matter of fact and also have its truth-value quite independent of fact.

Suppose one maintains instead that because the proposition expressed by the sentence "2 + 3 = 4" is true as a matter of fact, this suggests making the *sentence* express something which no fact will falsify—something Wittgenstein possibly meant. The sentence, "2 + 3 = 4," which is first understood as expressing a generalization about sets of things in juxtaposition or about the number one arrives at by counting a couple and a trio, is made to express something to which the behavior of sets of things or the experience one has upon counting the total group comprised of 2 + 3 objects is irrelevant. It is made to do this by being made to serve an entirely different purpose—to function as a rule for the use of the expressions "2 + 3" and "4." What once served to express a generalization which a different world could confute comes to serve as a guide in the conduct of language, proscribing such statements as "I put 3 apples into a bowl containing 2, but

[39] Lectures, 1934–35.

there were in all more than 4." The proscription, of course, is not of a falsity but of a use of language—of the use of "2 + 3 but not 4" to characterize any set of objects. What experience "suggests" is then the *choice* of language—because such a choice would be useful.

Does this description of the change in status of the sentence "2 + 3 = 4" explain the harmony between the arithmetic proposition and the fact that 4 is the result got by counting a couple and a trio? Does it explain the connection of the necessary proposition with its application? The difficulty I find is in specifying any connection between the sentence expressing a necessity and the empirical proposition originally expressed in the same words. When the truth of the proposition "2 + 3 = 4" was verified by experience, the expression "2 + 3 but not 4" at that stage had a use, whereas at the stage where the sentence is taken to express what is necessary this same expression is thereby denied a use. And this is to say that "2 + 3" has different uses, i.e., different meanings, at the two stages. "2 + 3" means something in the one case which is inconsistent with "not 4," and in the other case not. If the sentence "2 + 3 = 4" has different meanings at the two stages, what connection is there between the sentence for the necessary proposition and the observed fact that when a couple and a trio are counted the result is found to be 4 —i.e., with the fact which verifies the empirical proposition which the sentence no longer expresses?

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BOOK REVIEWS

Newton's Philosophy of Nature; Selections from his Writings, edited and arranged with notes by H. S. Thayer, Introduction by John Herman Randall, Jr. New York: Hafner Publishing Co., 1953. xvi, 207 pp. (The Hafner Library of Classics, No. 16.) Paper, $1.15; Cloth, $2.25.

The announced aim of this compilation is "to provide a wider representation of the interests, problems, and characteristically diverse philosophical levels and directions along which Newton's thoughts moved." In the selection and arrangement of materials, as in the Introduction and Notes, concern for Newton's "philosophy of nature" predominates over interest in the content of his science. The volume is useful in making some writings of Newton

more accessible, and the selected bibliography lists works helpful to their study. To be questioned is the representation which issues from the predominant concern as this operates both with respect to choice and ordering of materials and with respect to what is supplied in the Notes.

The aim of following out directions traveled announces either, or both, a historical and a logical development of Newton's thought. The arrayal of selections under headings is non-historical. Randall's Introduction, however, discourses about the setting and subsequent career of Newton's philosophy of nature, broaches some critical questions for history of philosophy, and points out the need to get behind Newton the symbol to the substance of his writings. Thayer's Notes also are mainly historical in their information, comment, and judgments. It is puzzling in view of this historical bracketing of the selected writings to find no attempt made to arrange any of the topically grouped selections in a temporal sequence. Such arrangement for a subject consecutively treated by Newton would have enabled readers to follow out one course of his thinking historically. Most suitable to this end, starting with Newton's *New Theory About Light and Colours* (his first communication to the Royal Society early in 1672), is the series of letters to Oldenburg in reply to objections raised by Pardies, Hooke, Huyghens, and Linus. Beginning with the first reply to Pardies in April, 1672, the correspondence continues until 1676 in defense of the theory on methodological grounds. Hypothesis and theory are differentiated, the procedure proper to experimental philosophy is specified, and reasons for not explaining the theory by hypotheses are developed.

It cannot be said of these letters, as Thayer asserts generally of Newton's writings, that they are "full of scattered remarks to the effect that hypotheses are to be avoided whenever possible." The considerations first introduced are amplified subsequently and with reference to preceding discussion. In a letter to Oldenburg dated Feb. 15, 1675–6, concerning those who object by propounding "an Hypothesis to explain my Theory," Newton writes: "I shall now for a conclusion remind you of what I have formerly said in general to the same purpose so that I may at once cast off all objections that may be raised in the future either from this or any other Hypothesis whatever." He asks Oldenburg to consider what had been said "both in my second letter to P. Pardies & in my answer to Mr. Hook, Sect. 4. concerning the application of all Hypothesis to my Theory. . . ." When, later, he continues to "decline" hypotheses in the *Opticks* and affirms his famous and much misunderstood "hypotheses non fingo" in the *Principia*

(2nd edition, 1713, Bk. III, General Scholium), he is carrying forward conclusions argued and reached in this series of letters. Having decided wisely to reprint the *New Theory About Light and Colours* in its entirety, Thayer would have done well to have followed it with the letters written in its defense, indicating the objections which had been raised to which Newton was replying and presenting in full all passages pertaining to method.

In saying that no attempt is made anywhere to have selections topically grouped appear in order of historical development, attention must be paid to a representation which Thayer makes about Section V: Questions from the "Optics." This section consists of the 31 Queries from the edition of 1730. Apart from corrections of *errata* noted in the edition of 1721 and addition in footnotes of citations from Newton's *Lectiones Opticae,* this fourth English edition is a reprinting of the third, which, in turn, is the same as the second English edition of 1718 except for minor additions. Of the Queries appearing at the end of the list of 31, Thayer writes as follows: "In an extremely condensed fashion, Newton appears here almost to be summarizing, in the actual order of their development, the fundamental problems and ideas which had occupied him most of his life." He mistakenly identifies these Queries, however, as having been added in preparing the work in 1717 for republication. The Queries numbered 25 to 31 in the second English edition appeared originally in an edition in Latin, the *Optice* of 1706, in which they were numbered 17 to 23 (only Queries 1 to 16 having appeared in the first edition of the *Opticks,* 1704). Newton renumbered these seven Queries to make place, in the second English edition, for eight new Queries numbered 17 to 24, and also expanded his statement of scientific method in Query 31. Encouraged, perhaps, by a misdating of the Queries that stand last, Thayer not only treats these as "almost" a summarization of "almost all of the fundamental ideas which occupied Newton"; he also alleges an "actual order of their development." If either was Newton's intention, no evidence is submitted to show that it was, nor is the allegation defended by showing how, in spite of exceptions that would have to be made, the sequence of problems and ideas taken up in the Queries can nonetheless be considered a reliable index to the chronological order of their development. Instead of viewing the Queries as a summarization, there is reason in Newton's own words (not included by Thayer) for viewing them as unfinished business bequeathed to posterity, *viz.*, "When I made the foregoing Observations, I design'd to repeat most of them with more care and exactness, and to make some new ones for determin-

ing the manner how Rays of Light are bent in their passage by Bodies for making Fringes of Colours with the dark lines between them. But I was then interrupted, and cannot now think of taking these things into farther consideration. And since I have not finish'd this part of my Design, I shall conclude with proposing only some Queries in order to a farther search to be made by others." The Queries eventually totaling 31 then follow.

Turning from these historical considerations to the attempted logical ordering of selections under topical headings, we have four main groupings besides the fifth already noted. These are I: The Method of Natural Philosophy; II: The Fundamental Principles of Natural Philosophy; III: God and Natural Philosophy; and IV: Questions on Natural Philosophy. Cross-references are supplied to mitigate segregation which places under one classification passages relevant under another. These references are on the skimpy side but are supplemented by the Notes and can further be amplified by a reader taking the trouble to collate passages. Of more moment, however, is skimping with respect to amount of space accorded some topics and with respect to omission of others.

The first kind of paucity is found in the section on method. Including a cross-reference to passages in the *Opticks,* Queries 28 and 31, the total amount of text runs to five and one-half pages. More is addible to this by searching out other passages included in the volume. Even so, the selections are altogether inadequate to do justice to the range, content, relationships, and acumen of Newton's thinking about problems of method. Why, for example, does Thayer publish (although not in the section on method) Newton's reply to one of Hooke's two principal methodological objections against the *New Theory About Light and Colours* and omit the other contained in the same letter to Oldenburg dated July 11, 1672? Hooke objected, "I cannot think it to be the only hypothesis." Newton replied, "Had I intended such hypothesis, I should somewhere have explained it. But I know that the properties, which I declared of light, were in some measure capable of being explicated not only by that, but by many other mechanical hypotheses; and therefore I chose to decline them all." He goes on to acknowledge that he argues the corporeity of light from his theory; "but I do it without an absolute positiveness, as the word *perhaps* intimates, and make it at most but a very plausible consequence of the doctrine, and not a fundamental supposition, nor so much as any part of it."

The letters and portions of letters assigned to topics constitute good choices in what appears, but the dismembering of letters

has two undesirable consequences. The first, just noted by example, is the impoverishment of representation by omission of a part of a letter which has no surrogate in the volume to which one can turn to find just the reasoning set forth. The second consequence, also illustrated by Thayer's excerpting from this same letter to Oldenburg, is impoverishment of representation by removal of an excerpt from a context in which it both contributes and gains significance in connection with other discussion. The portion selected by Thayer and published in Sect. III, 2, "On the Science of Colors," is Newton's reply to Hooke's objection to a mathematical certainty of the science of colors which Hooke supposes Newton to be affirming in saying that the science is mathematical. In the reply, designated as "interesting remarks" by Thayer, Newton writes: "Now the evidence by which I asserted the propositions of colors is in the next words expressed to be from experiments and so but physical, whence the propositions themselves can be esteemed no more than physical principles of a science. And if these principles be such that on them a mathematician may determine all the phenomena that can be caused by refractions . . . ,I suppose the science of colors will be granted mathematical and as certain as any part of optics. And this may be done, I have good reason to believe, because ever since I became first acquainted with these principles I have, with constant success in the events, made use of them for this purpose." The portion excerpted by Thayer, when read in context, contributes to the elucidation of theory as differentiated from hypothesis and, in turn, gains in significance from the primacy assigned to theory over hypothesis. If the reason for setting this portion by itself apart from the section on method and out of textual and historical setting springs from the scheme of classification employed in arranging selections, this is reason also for being dissatisfied with it.

Where omissions and scatterings of related discussions make it appear that Newton gave much less attention to method than he actually devoted, the abundant presentation of speculations on God makes it appear that he was much more given to metaphysical and theological disquisitions than is in fact characteristic of him in temper and body of his philosophizing. The places and occasions of these speculations receive attention in Thayer's Notes, but critical comment about their prominence is needed to correct the distortion resulting from the biased selecting of writings exhibited in the starving of the section on method to feed the section on God.

Omission of mathematical writings and mathematical demon-

strations is quite defensible in keeping with the announced aim of the book and its intended audience. Hardly defensible, however, is neglect of passages in which Newton discusses or develops the analogy between physical and mathematical analysis, the relation of sensible measures to rules of measures, and, more generally, the relation of "experimental philosophy" to the "mathematical way" of reasoning in natural science. This neglect is found in the omission of pertinent materials, e.g., the defense in the *Principia* of the intelligibility of an "ultimate proportion of evanescent quantities," and the discussion in the *Opticks* of new theorems for "mathematically determining all kinds of Phaenomena of Colours." It is found also in the absence of topical headings for the grouping of selections appropriate to represent the central importance which these relations had for Newton and have for our comprehension of his thought.

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A Foundation of Ontology; a Critical Analysis of Nicolai Hartmann. Otto Samuel. [Translated by Frank Gaynor.] New York: Philosophical Library [1953]. xv, 155 pp. $3.75.

The announced purpose of this book is to "render a good service to Americans interested in philosophy by trying to familiarize them with the work of Nicolai Hartmann" (who "must not be confused with Eduard von Hartmann"!). The odd-numbered chapters are outlines ("digests") of Hartmann's *Zur Grundlegung der Ontologie,* while the even-numbered chapters contain critical comments of the author in the form of "a friendly debate with Nicolai Hartmann." The over-all effect is that of a sort of philosophical club-sandwich.

The outlines attempt to condense into approximately 67 pages Hartmann's extensive exposition of the problems of ontology (in 322 pages of twice the size), e.g., the relation of essence and existence, the modalities, ideal and real existence, etc. This procedure has not only resulted in omitting the wealth of detailed discussion which is the principal value of Hartmann's book, but the almost random selection is unlikely to give to the reader any conception of Hartmann's general approach to these problems. Moreover, the account is so far from intelligible that I found it impossible to understand these chapters without constantly referring to Hartmann's *Grundlegung* itself. It is doubtful whether a "guidebook" (p. xv) of this sort will succeed in conveying the sense of the original any better than the perusal of a Baedeker would "familiarize" us with the contents of the Vatican.

The critical chapters maintain that Hartmann's theory is "one-sided" and only "tells half the story," because he seems "to have whole-heartedly embraced transcendent realism" (p. 7). Thus by changing "the whole of cognition into a flatly transcendent-realistic relationship . . . he misses entirely the immeasurable depth, beauty and fecundity of transcendental idealism" (p. 8). The principal error attributed to Hartmann is his treating Being solely in terms of the "Extant" (*das Seiende*), which leads him to neglect the "transcendental aspects of Being" and especially the "concrete Nothing," which is the "Nothing of Nothing" (p. 150). The author recommends the recognition of the negative aspect of Being which would permit "the union of the ontological transcendental-idealistic line with the ontical transcendental realistic line in meontology" (p. 149).

The difficulties in following both the outlines and the criticisms are not alleviated by the rather awkward translation. It seems evident that the translator, Mr. Frank Gaynor, is not familiar with conventional philosophical terms in English, for he goes out of his way to coin scores of neologisms of his own, e.g., "factuality" (for *Wirklichkeit*), the "Extant" (for *das Seiende*), "contentual," "scepsis," "paradoxon," etc. This seems unnecessary since Hartmann's philosophy, in contrast to that of the existentialists, is concerned with traditional problems and concepts familiar to the English reader—a fact which should make his writings easier to translate than those of almost any other contemporary German philosopher.

Any reader desiring to obtain a "bird's eye" view of Hartmann's ontology would do better to read his recently translated *New Ways of Ontology*.[1]

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Der Mensch und die Natur. THURE VON UEXKÜLL. Bern: Francke Verlag [1953]. 270 pp. (Sammlung Dalp, Bd. 13.) Sw. fr. 7.80.

This book offers a Kantian-Pragmatic approach to the many facets of one of today's major philosophic problems—the relation of man to nature.

Following Kant, Uexküll takes the view that the knowing subject confronts "nature-in-itself" through forms which are not part of nature-in-itself but which represent only the perspective of the knowing subject. From Francis Bacon he

[1] Reviewed in this JOURNAL, Vol. LI, no. 3 (February 4, 1954), pp. 108–112.

adopts the pragmatic notion of the dependence of knowledge on behaviorial capabilities. Both Kantian and pragmatic strands are given a biological interpretation, and the product is brought to bear on problems traceable to dualistic elements in present-day thought. "Nature-in-itself" or *Vorwirklichkeit* is equated with traditional "primary matter." The "boundaries" (*Grenzen*) of temporal "slices" (*Ausschnitten*) of reality encountered by the subject are equated with traditional "form"; and realizing itself within these forms is "substance"—which constitutes a union of form and primary matter.

The slices of reality encountered by the subject as well as their boundaries are dependent upon the "set" or goal of the subject. In addition, the confrontation of a particular slice of reality with particular boundaries postulates an outcome for the slice—an end, the temporally prior segments of which may be regarded as means to this end. It is therefore apparent that in Uexküll's view, a correct understanding of the teleological aspects of man and nature is important for a proper grasp of their relation.

Uexküll's approach to teleology is valuable in many respects. To begin with, his biological orientation leads him to reject the idea that there must always be a conscious envisagement of purposes. As he points out, many confusions result from the contrary suppositions, including the mistaken belief that the ascription of teleology to non-human occurrences necessarily involves anthropomorphism. Then, distinction is made between "other-serving" (*fremddienlicher*) and "self-serving" (*selbstdienlicher*) purposeful activity, and the relation of the concepts of cause and effect to those of means and end is considered. The direction and control exerted on activity by purposes is taken note of by comparing their operation with the psychological concept of "set" (*Stimmung*), and the element of choice involved is pointed up by the definition of a purpose as "that which itself has the capability of choosing the means to its own realization" (p. 108). Finally, the connection of teleology with perception is recognized in an investigation of the relation of the threshold concept (*Schwelle*) to purposiveness.

In my opinion, one of the virtues of Uexküll's book is that he recognizes the existence of teleology on the physical-chemical level as well as on the biological level, thus baring a kinship of man with nature not generally supposed to be present. Unfortunately, however, use of Kant's distinction between "constitutive" and "regulative" concepts serves to dissipate the ameliorative effects of this recognition. In the biological realm, teleology is admitted to be constitutive: biological beings themselves ***really*** behave in a

purposeful manner. But on the physical-chemical level, teleology is regarded as only regulative: physical-chemical phenomena are purposeful only insofar as this interpretation is put upon them by the observer. In refutation it might be said that Kant's distinction seems arbitrary. To cite just one instance, it would not appear to be applicable to modern feedback mechanisms.

In general, *Der Mensch und die Natur* suffers somewhat from other similar defects of its Kantian strain—defects which have a faint taste of the very dualism it seeks to remedy. It nevertheless remains a comprehensive and interesting book.

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NOTES AND NEWS

The following officers were elected for the year 1955 at the meeting of the American Philosophical Association, Eastern Division, in December, 1954:

Albert Balz, President.
Paul Weiss, Vice-President.
Lucius Garvin, Secretary-Treasurer.
Victor Lowe, John Wild } new members of the Executive Committee.

Copies of the volumes containing the symposium papers presented at the meetings of the Eastern Division of the American Philosophical Association in 1952 and 1953 (Volume I, *Science, Language, and Human Rights;* Volume II, *Academic Freedom, Logic, and Religion*) are still available for purchase from the University of Pennsylvania Press, 3436 Walnut Street, Philadelphia, Pennsylvania. The price to members of the profession is $2.20 per volume.